Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-772-AC2, 2019
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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Droplet Clustering in Shallow Cumuli: The Effects of In–Cloud Location and Aerosol Number Concentration" *by* Dillon S. Dodson and Jennifer D. Small Griswold

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Received and published: 23 February 2019

REPLY: We would like to start by addressing concerns raised by both of the Referees. First, we apologize for the delayed response and posting of the revised manuscript. We put in a great deal of time and effort to do a rewrite of the paper, to shift the focus from the smaller scale clustering originally discussed to larger scale mixing as a result of entrainment. Both Referees raised concerns that the PCFs examined are more closely related to entrainment mixing as compared to preferential concentration, and we have also come to that conclusion. In particular, our PCF spatial scale does not extend into the Kolmogorov range, suggesting that inertial clustering (if present at all) isn't being

measured. Our PCF curves also mirror those presented in Good et al. (2012) and Ireland and Collins (2012), which used the PCF for the purpose of analyzing larger-scale clustering due to entrainment mixing. Lastly, we have de-normalized all PCF curves displayed throughout the manuscript. In Saw et al. (2012) the PCF curves are normalized at the range the larger scale inhomogeneous mixing is occurring (i.e., the 'shoulder' region of the curve). Since the 'shoulder' region of the curve is what we are analyzing, normalizing it to a common value becomes unreasonable.

COMMENT: The paper is on analysis of field experiment data pertaining to droplet clustering in clouds. This is an important subject considering that the experimental evidence for preferential concentration (inertial clustering) due to interaction with turbulence has been well established in the laboratory since at least a decade ago, yet it has never been satisfactorily documented from field data of atmospheric clouds (as far as I know). The main difficulty stems from statistical non-stationary commonly encountered in cloud data, as the author have discussed in the current manuscript.

The methodology and the foundation of such practice, the authors failed to disclose, has evolved and, in my view, sufficiently matured in the course of time since Shaw et al. (2002). A mathematical foundation (or origin) of the multiplicative effect of larger scale inhomogeneity on small scale clustering signatures in the Radial Distribution Function (by definition = PCF+1) was exposed (if not proved in strict mathematical sense) in Saw et al. (2012); the same paper also includes a demonstration, using experimental data, of how large scale inhomogeneity resulting from incomplete turbulent mixing caused an apparent uniform upward "shift" of the inertial clustering signatures at small scales. In short, it was shown that the mathematics of how RDF is calculated dictates that: when two concurrent phenomena of inhomogeneities are uncorrelated and occurs with sufficient scale separation, the plainly calculated RDF equals the product of two RDFs, each results from the one of the two phenomena acting alone, i.e. g(r) =g_1(r) * g_2(r).

REPLY: We do believe a better job could have been done in explaining the normalization process due to the confusion that has been shown. Although not discussed or included in the revised manuscript, originally we believe the process by which we did the normalization was flawed. In particular, we were normalizing the PCF curves at the largest scales where the PCF continues to decrease sharply (the region directly after the shoulder region) in order to analyze the differences in the shoulder region. However, in Saw et al. (2012) the PCF is normalized at the shoulder region in order to compare differences in the inertial clustering. We are unaware of any work that has been conducted which shows PCF normalization is valid at the largest scales in order to analyze the shoulder region scale clustering. Although it has been shown that the PCF can be normalized in the shoulder region to analyze inertial clustering, normalizing the PCF at the largest scales to analyze the entrainment scale clustering may or may not be valid. Although this in and of itself could be an interesting project for future thought. Since we are no longer performing the normalization, we do not mention anything about this within the new manuscript.

COMMENT: The above discussion, while relieving the doubts on the normalizing practice, unfortunately triggers another conundrum. Referring to the results (e.g. Figure 4) in Saw et al. (2012), the RDF signature associated with mixing of large (above Kolmogorov) scale inhomogeneity is a curve monotonic increasing with decreasing scale at first, transitioning into a plateau as we approach dissipative scales (the height of the plateau gives a relative measure of the level of inhomogeneity observed). The shape of the corresponding PCF would be qualitatively similar (but somewhat stretched at the large scale end) and this is remarkably similar to all of the PCF seen in the current paper. While, on the other hand, the RDF signature associated with inertial clustering, as found in many previous studies (many direct numerical simulation studies, theories and some experiments e.g. Saw et al. (2012)), have shapes suggestive of power-laws. Thus, it is reasonable to ask if the resulting PCFs in this paper should be more appropriately associated with the inhomogeneity at cloud edges due to entrainment and thus mixing dry and cloudy air, rather than with inertial clustering? Such a view is not inconsistent with the authors own estimate of Stokes numbers of the observed droplets, which is on the order of 0.01 (although we don't really know how good is the

100cm2 s-3 guess for the energy dissipation rate). Previous works suggests that St Order(0.01) should results in RDF with power-law exponent of order 0.01 or less, very likely indistinguishable from a plateau or flat horizontal line, given realistic signal-noise ratio of in-situ cloud measurements.

REPLY: As already mentioned, we believe you are correct in stating that the PCFs in this paper should be more appropriately associated with the inhomogeneity at cloud edges due to entrainment and the subsequent mixing of dry and cloudy air, as opposed to inertial clustering. An explanation of the PCF curve and how it should look for inhomogeneous data (as you state above) is given in the new manuscript on page 6 lines 13-21, and this is also conveyed on page 7 lines 9-12 in describing the PCF for real data calculated in Figure 1.

We also agree that any inertial clustering that is present would be indistinguishable from the noise the current PCFs display. But it is believed that the PCF scale we currently use does not extend to small enough scales to even measure inertial clustering anyways (we would need to measure down to mm, whereas we currently only extend down to $\sim$ 3 cm). Lehmann et al. (2007) does not record an increase in the PCF due to inertial clustering for cumuli until length scales of approximately 1mm. We do not extend to the mm scale because we would need to reduce the dt used to calculate the PCF (see page 6 line 24-26), which would result in an even noisier PCF than what is already displayed.

COMMENT: I think major addition/revision is needed to convincingly disentangle mixing signatures from inertial clustering signature, otherwise the paper might have to be rewritten with a different focus (see below).

In relation to points above, perhaps I would suggest, if I might be so bold, that the current data might be better used for careful study of the RDF signatures of the cloud edge entrainment-mixing, which is perhaps also of interest? A good understanding of this will certainly be informative for discovery of proper methods of disentangling entrainment-mixing from inertial clustering in cloud data.

REPLY: As you have suggested, we have completely rewritten the paper to focus on clustering as a result of entrainment mixing as opposed to inertial clustering. We believe that focusing our results on entrainment provides a better analysis and explanation of the PCF signatures that are shown throughout the manuscript.

Although the most significant challenge in all field studies of clouds is distinguishing clustering due to droplet inertia from clustering due to entrainment and mixing (Lehmann et al. 2007), I don't believe the PCF curves displayed here would be appropriate for disentangling entrainment-mixing from inertial clustering since our curves do not extend down into the Kolmogorov range. The fact that our curves don't include inertial clustering also makes them ideal for examining the clustering effect due to entrainment, since we don't have to worry about the Kolmogorov scale effects.

COMMENT: minor comments: page 4, line 31: replace "by" with be page 8, line 1: time should have units in seconds not meter.

REPLY: The section of text that originally included page 4, line 31 has been removed. Units have been changed from meters to seconds (see page 7, line 31).

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Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-772, 2018.